THE METHOD OF CELL DIVISION IN MONIEZIA.

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Since the appearance of Richards's paper on the method of cell division in *Moniezia* (Richards, '11) Dr. Richards, in answer to a request of mine, has very kindly permitted me to examine some of his slides of early stages in the development of the ovary and of cleavage stages, with the privilege of publishing the results. The present paper consists of a brief statement of the results of my examination of these slides, together with some consideration of recent criticisms of my observations on amitosis. I take this opportunity of acknowledging my indebtedness to Dr. Richards and also of acknowledging an error of my own which though it does not directly concern the chief point at issue certainly requires correction. Dr. Richards is undoubtedly correct in his statement that the early cleavages in *Moniezia* are mitotic, either entirely, or to a much greater degree than my earlier statements would indicate. With this acknowledgment of my error the chief point of disagreement between Richards's positive observations, so far as they go, and my own is removed. The difference between us seems to me to rest now on Richards's failure to recognize, or to interpret as I have done, what the material actually shows.

The whole problem of amitosis seems to me somewhat unprofitable at present as a subject for investigation on a purely observational basis. Experimentation is necessary before we can reach any certain conclusions as to the significance of either mitosis or amitosis in the life of the cell. Moreover, cytological thought is at present dominated by morphological hypotheses, which are themselves based on direct observation rather than on experiment. So long as this remains the case all observations which do not accord with the current hypotheses will be explained away by one course of reasoning or another and brought into agreement with these hypotheses. So long then as our conclusions are based simply upon observation in fixed material and without any

attempt to control conditions in that material there will always be room for endless discussion and controversy. It seems to me highly improbable that cytology will make any very great real advances in this direction. But when cytologists shall seriously devote their energies to the problem of the physiology of the cell, a field of investigation that has been entered only here and there, and to the control of cellular phenomena, then I believe we shall see real progress, together with the consignment to oblivion of some of the hypotheses now most fondly cherished.

As regards *Moniezia*, however, I have at present nothing to offer except further observations, or rather my own interpretation of what I have seen in Richards's material as against his interpretation of what he has seen. Beyond the present paper I do not propose to enter into any further consideration of the problem of amitosis until I can do so on a different basis, as I hope to be able to do at some time in the future.

Thus far I have only attempted to defend my own observations and conclusions against what seemed to me unmerited criticism and skepticism based on insufficient data. Richards's paper on *Moniezia* is, however, a real reinvestigation of the subject and as such I can only welcome it and accord it the consideration which is due to any piece of scientific work, although I am compelled to criticize it in various points. The present paper includes a statement of the results of my observations on Richards's slides, together with some discussion of his arguments and conclusions and brief reference to criticisms offered by other authors and to the validity of cytological theory.

The slides contain exactly the same pictures as my own. There is no question here of differences of fixation or of staining. If amitosis occurs in my material then it occurs in Richards's in the same regions of the body and the same stages of development.

I. The Primordia of the Ovaries and Ducts.

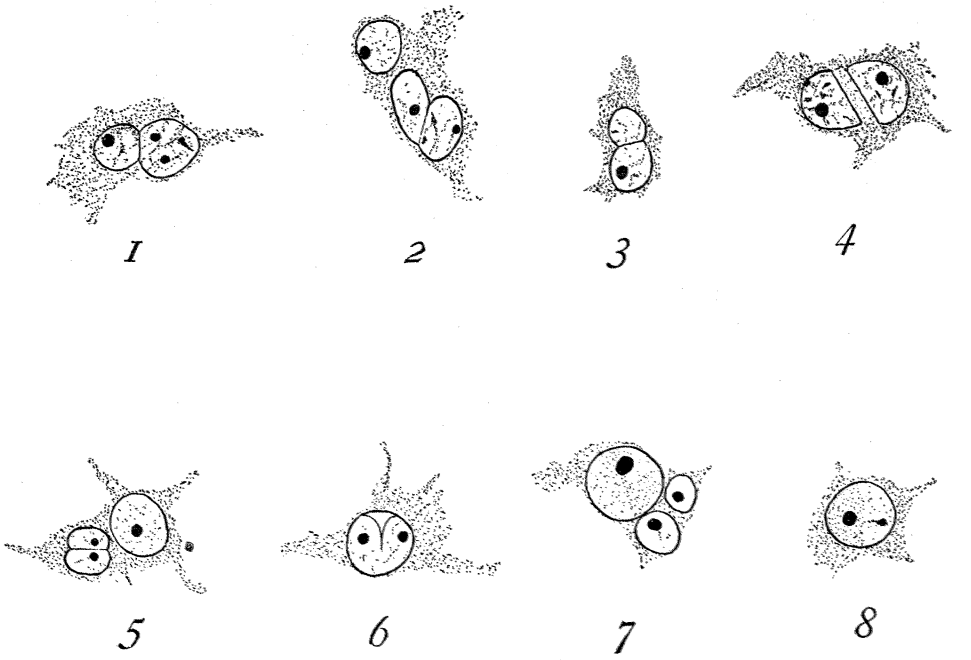
Figs. 1–8 are camera drawings from the primordia of the ovaries in Richards's slides. They were drawn from several slides with different methods of fixation and staining. In the figures I have attempted to reproduce what I have seen as

exactly as possible, though this is a somewhat difficult matter as regards the extranuclear phenomena. Godlewski ('09) has recently objected to my earlier figures on the ground that they were too highly schematized. This objection seems to me to be based largely on unfamiliarity with the material. In the early stages of the gonad visible cytoplasmic boundaries are often entirely lacking and the question as to whether cytoplasmic division follows nuclear division has no point, since it is quite impossible in most cases to distinguish cells. In later stages the germ cells do become distinct, but in those stages amitosis does not occur or occurs but rarely. In my earlier work I attempted to reproduce the essential features of the nuclei as exactly as possible. The only schematization consisted in not attempting to indicate an intranuclear achromatic structure except where such a structure was clearly visible and in leaving out various extranuclear details which did not bear upon the problem under consideration. In many of the nuclei the karyosome alone is stained and the rest of the nucleus appears almost or quite homogeneous; in other cases a more or less distinct structure of strands or an apparent reticulum does appear. In cases where I could not see such a structure distinctly I did not attempt to represent anything in the nucleus except the karyosome. It seems at least probable that the apparent achromatic nuclear structure is in many cases simply an artifact due to the action of the fixative. In the figures of the present paper I have represented a vague structure in all nuclei, but for most of the nuclei this must be regarded as little more than an attempt to show that something is there.

Richards's statement (p. 135) that the parenchyma is not a syncytium seems to me to require qualification. It may well be that it is not a continuous syncytium throughout the whole proglottid in later stages, but there cannot be the slightest doubt that several or many nuclei are often found imbedded in a more or less continuous mass of cytoplasm. This is true of the early stages of the gonads as well as of other parts. Where the nuclei are far apart they are usually, as Richards states, surrounded by more or less definite and distinct cytoplasmic masses, but this is certainly not true in cases where they lie close together.

Fig. 1 shows two nuclei in close contact, with the surfaces of contact flattened. If each nucleus surrounds itself with cytoplasm it is difficult to understand how a condition like Fig. 1 can arise except by division. Moreover, this is not the usual result of mitotic division in *Moniezia,* nuclei which arise by mitosis are separated by an appreciable distance when they form and are usually of equal size, which the two nuclei in the figure are not.

Fig. 2 shows a case which I am unable to interpret except as a direct division. The most careful focusing shows that the nuclei



are in the same plane and that the membrane separating them is not a strand of "linin" but a real membrane. Moreover, this membrane is apparently thinner and more delicate than other portions of the nuclear membrane.

In Fig. 3 two nuclei are shown which seem to be just completing division, much as in Fig. 1.

Fig. 4 shows two very distinctly hemispherical nuclei with flattened surfaces parallel and separated by a small amount of

cytoplasm which stains less deeply than other parts. They appear as if division had recently occurred and cytoplasm had begun to form on the adjoining surfaces. If it were not for certain preconceived ideas concerning the method of cell division, I think no one would hesitate to regard such a case as a very strong indication—I do not say proof—of direct division.

In Fig. 5 the apparent division of a small nucleus is shown. The membrane between the two parts is more delicate than the rest. The position of the two "nucleoli" is important. They are close to the separating membrane and directly opposite each other. This position is of very frequent occurrence in such cases, far too frequent I believe, to be the result of chance. It suggests very strongly that division of the nucleoli may occur in many cases.

Fig. 6 is a case of apparent "endogenous" division. Richards (p. 14) was unable to find anything of this kind in the material. I found a number of such cases in his slides.

Figs. 7 and 8 show cases in which the two halves of a nucleus are slightly different in color, a phenomenon of not infrequent occurrence, which Richards also failed to observe. I do not know, of course, that such a condition indicates division, but I regard it as highly suggestive in that it certainly indicates some difference in condition in the two parts of the nuclei and a certain degree of independence of each other.

Figures of this sort might be multiplied indefinitely from Richards's slides, but there is no reason for giving others here. My chief object is to show that exactly the same pictures appear in his material as in my own. I am perfectly well aware that none of these figures and likewise none of my earlier figures constitute a real demonstration of the occurrence of amitosis, for such a demonstration is impossible in fixed material.

Numerous similar pictures were found in the early stages of the genital ducts, the vitellaria and the parenchyma during the stages when increase in the number of nuclei is occurring in these regions. There is, however, no reason for giving figures from these organs, for on the basis of current cytological theory the point of chief importance is whether amitosis occurs in the germ cells. If its occurrence is admitted for these cells, most

cytologists would, I think, be willing to admit at once that similar phenomena in somatic cells doubtless indicate its occurrence there.

We may now consider the value of such pictures as evidence and the force of some of the criticisms made by Richards and others.

As regards the first point, I have observed pictures of this kind only in regions where the nuclei are increasing in number. Godlewski ('09) has suggested that I may have mistaken nuclear fusions for divisions. It would be strange indeed if nuclear fusion should occur only or chiefly in regions where the nuclei are undoubtedly increasing in number in some way.

Boveri's suggestion ('09) that the two nuclei in a cell represent the paternal and maternal chromatin refers primarily to the amphibian cells and perhaps he himself would not regard it as adequate to account for the appearances in *Moniezia*. Certainly there is no evidence of the separation of the two chromatins in mitotic division in *Moniezia*, even in cleavage. As regards the amphibian cells which I figured in an earlier paper (Child, '07*c*, Figs. 10 and 11) it should be repeated that the stages studied were not cleavage stages, but mostly larval stages, in which I could find no evidence, and I took especial care concerning this point, of a separation of paternal and maternal chromatin in mitotic division. I do not believe that this suggestion will account for the phenomena even in *Amblystoma* and it certainly will not in *Moniezia*.

As regards the possible increase of the nuclei by migration in particular regions, *e. g.*, the primordia of the gonads, Richards himself admits (p. 139) that the evidence for migration is not strong. Moreover, even if migration does occur in the formation of the gonads, there must nevertheless be a considerable primary increase in the parenchymal nuclei before the gonads are formed. Richards (pp. 138–9) finds the actual increase in parenchymal nuclei to be about equal to the increase in volume of the proglottids. Such an increase is, I believe, entirely inadequate to account on the basis of migration for the very large number of nuclei which appear in gonad formation.

Concerning the occurrence of mitosis Richards's actual ob-

servations are essentially identical with my own; he finds it but rarely, in fact he has not figured anywhere a case showing so many mitoses as my Fig. 8 (Child, '07*a*), in which seven cases of mitosis are shown. His argument concerning the ratio of resting to dividing cells in sections (pp. 139–40) concerns matters of which we really know almost nothing one way or the other; moreover, I am fully convinced that in many cases besides *Moniezia* where the infrequently observed mitoses have been supposed to account for all nuclear increase, numerous amitotic divisions actually occur. But besides all this, my conclusions concerning *Moniezia* are not based primarily on the frequency or infrequency of mitosis, but upon the actual observation of what I cannot regard as anything but amitosis. The infrequency or apparent absence of mitosis in many regions of *Moniezia* was what first led me to undertake a further investigation of the material and the remarkable infrequency of mitosis according to all observers in the neck region, the developing gonads and the genital ducts still seems to me to be highly suggestive, especially when we add to this fact the very frequent occurrence of what seems by all criteria at present available to be direct division.

Concerning periodicity in mitosis (Richards, '11, p. 140) the only positive observation which suggests anything of the sort is mine, which was cited in an earlier paper (Child, '10). This was a case of scolex, neck region and a few proglottids in which most of the nuclei were dividing mitotically. I have no means of knowing whether this is a normal occurrence, but if it is, it seems probable that this specimen is a young scolex, for the nuclei throughout the scolex are dividing as well as the others and these nuclei certainly do not take part in the formation of proglottids in later stages. I have considerable evidence for various forms which seems to me to indicate that many organisms or parts may start a period of nuclear multiplication by mitosis, while in later stages amitosis becomes increasingly frequent and if this scolex is normal it may be a case of that kind. It is quite possible, however, that it may be the result of certain special conditions of which we know nothing and not a normal stage of development in *Moniezia* at all.

It is certainly most strange, if mitosis occurs periodically or in waves as Richards suggests, that neither Richards nor Young nor I—except in the case above mentioned—have happened to observe any of these waves of mitosis in the numerous animals that we have sectioned. All the evidence which we have obtained indicates a remarkable infrequency and a scattered occurrence of mitosis.

But the argument based on mitosis is not direct evidence for or against the occurrence of amitosis. When we find the pictures which I have figured occurring frequently and confined to regions in which the nuclei are known positively to be increasing in number in some way, the direct evidence for the occurrence of amitosis seems to me to have far greater value than any indirect evidence based on the frequency or infrequency of mitosis.

On pages 149–50 Richards argues that in the primordium of the ovary and ducts the region of nuclear increase is at the periphery and states that it is here that mitosis is chiefly found. With the latter statement I agree, but I find it difficult to understand the grounds for his conclusion that nuclear increase is confined to the periphery. Certainly the increase in the number of nuclei at the periphery is not very great, while in the central region of the primordium the nuclei are very numerous, crowded close together. Moreover, these central nuclei are in early stages very much smaller than the peripheral. Unless there has been extensive migration from the periphery toward the center it is impossible to account for the great number of nuclei there except by proliferation. Richards has failed to find amitosis in this region, but I have found numerous cases of what I regard as amitosis in his slides. He maintains that the central nuclei are beginning differentiation (p. 150). I have been unable to observe any visible differentiation in this region at these stages and besides these crowded nuclei are on the average much smaller than any nuclei of the whole primordium after the visible differentiation has begun. In later stages, when, as I believe, proliferation becomes less rapid, these nuclei gradually enlarge again and become more widely separated and then visible differentiation begins. For these reasons I am unable to agree with

Richards that the region of greatest proliferation is the periphery of the primordium. Such a conclusion fails, it seems to me, to take account of all the observed facts. The great number, the very small size, the crowded condition of the nuclei, the relatively small amount or cytoplasm and the very frequent occurrence of apparent amitoses force me to maintain my original position, that the central region of the primordium is a region of more rapid proliferation than the peripheral. In connection with this point I have evidence from various species that in regions of more rapid proliferation amitosis is more frequent, while in regions of slower proliferation mitosis may be the chief or only method of division.

On p. 150 Richards states that "the method of cell multiplication in the female sex ducts of *Moniezia* cannot at the present time be positively stated." Here evidently the author has lost faith to some extent in his own argument, for the conditions in the ducts are not essentially different, so far as observation shows, from those in the primordia of the gonads themselves.

Of the possibility of observational error (Richards, '11, pp. 140, 141, etc.) no one can be more keenly aware than myself. I can only state again that I believe that I have used every precaution possible in direct observation of fixed material. I have often spent hours on a single case with the most careful focusing, changes of illumination with the aid of diaphragm and mirror and the alternating use of artificial light and sky light with and without color screen. Until Richards can show that he has taken at least equal precautions before concluding that direct division is absent, he lays himself open to the charge of error or superficial observation.

As regards the difficulty of observation in the small and often crowded nuclei of cestodes, a point which Richards emphasized in his earlier paper (Richards, '09), it is of course true that the nuclei are small and often crowded, but if one devotes sufficient time and care to the matter it is possible to find nuclei which show apparent direct division with almost diagrammatic clearness and I have seen many such, both in his material and in my own. In such cases I have not been able to convince myself by any means which I could devise that the appearance

of direct division was a deception or an artifact, or that it lay within the limits of observational error, as Richards asserts for all apparent cases of the sort observed by him.

It would be of interest to know how far Richards is familiar with the appearance of amitosis in forms and tissues where it is generally admitted to occur. I have spent considerable time familiarizing myself with such material and I find many cases of apparently dividing nuclei in *Moniezia* which seem to me to be identical in appearance with cases of division in such tissues.

And finally, Richards admits finding numerous "paired nuclei" and nuclei between which no layer of cytoplasm could be distinguished; it is of interest incidentally to note that his statements on this point concerning the ducts and vitellaria are much less cautious than those concerning the ovarian primordium itself. And although both he and I have observed mitosis in the ducts, he is willing to admit concerning them that there is "no certain evidence for amitosis and that for mitosis is, perhaps, insufficient to account for the growth which has taken place" (p. 150). And concerning the vitellaria he says, "if mitosis is not clearly proved as the sufficient cause of cell multiplication, amitosis is certainly less so." Concerning the ovarium primordium his statements are much more guarded. Here the influence of cytological hypotheses appears. Most authors are willing to admit the occurrence of amitosis in somatic cells, though they would regard as rank heresy the assertion that it occurs in the germ cells. Certainly *Moniezia* affords no basis for such a distinction and as regards many other forms we have not the slightest evidence that the germ cells in early stages are different in any way from somatic cells. The most that observation can tell us on this point is that in some forms they appear early, in others late.

As to what constitutes evidence for amitosis Richards does not seem to be entirely clear. As regards the vitellaria he says "but for amitosis there is only negative evidence" (p. 147), yet in the same paragraph he admits that "the arrangement of nuclei in pairs is, perhaps, more in evidence here and indented nuclei are somewhat more numerous." These observed facts represent conditions which we should expect to find present if

amitosis occurred and therefore constitute not negative but positive evidence of considerable value. They do not, of course, constitute a demonstration, but, as I have pointed out above, an actual demonstration or proof is impossible in fixed material. To my own mind, all the facts concerning which Richards and I agree, together with the cases which I regard as nuclear division, but which Richards has failed to find, constitute a practical, though not a logical demonstration of the occurrence of amitosis in *Moniezia*. On p. 127 Richards asks "whether the failure to find evidence of a certain process, using proper methods and exercising due diligence, is not positive evidence of the lack of that process?" Such a question can of course only be answered in the affirmative, but this does not constitute a refutation of my statement that Richards had presented only negative evidence for the occurrence of amitosis; it is of course merely the same statement in other words. And we are all aware that positive evidence of the absence of something must be of the strongest character before it can be regarded as refuting direct positive evidence of the presence of the same thing. Richards is unable to show with anything approaching certainty that mitosis is the only method of cell division in the ovarian primordium. I have presented positive evidence that amitosis occurs in addition to mitosis, though my evidence does not amount to an actual demonstration since that is impossible. I believe, however, that the evidence for the occurrence of amitosis in the ovarian primordia of *Moniezia* is almost if not quite as conclusive as that for its occurrence anywhere else in other species.

II. The Cleavage Stages.

As regards the cleavage of *Moniezia*, my examination of Dr. Richards's slides enbles me, as mentioned above, to correct the error into which I fell in maintaining that "cases of mitosis are rarely seen after the first cleavage, but amitosis is of frequent occurrence" (Child, '07*b*). Richards's observations as stated in his paper led me to believe that I must have been in error on this point and the examination of his slides removed any doubt which might have remained.

There is no question but that the earlier cleavages beyond the

first were almost entirely absent from the proglottids on which most of my study of cleavage was made and that my Figs. 21–26 (Child, '07*b*) are in reality sections of considerably later cleavage stages than I had supposed.

The passage of the eggs into the uterus in *Moniezia* undoubtedly occurs periodically and as the eggs are fertilized in the course of this passage it follows that in any single proglottid containing cleavage stages not all the stages will be present. In the proglottids on which I based my study of cleavage it happened that first cleavages were numerous, *i. e.*, a period of passage of eggs from the ovary to the uterus had occurred recently. But as a reexamination of these slides after my study of Richards's material, shows, the stages between the first and occasional second cleavages and much later stages were almost entirely absent. My figures of supposed early cleavages are in reality sections of eggs containing a considerable number of nuclei, but the plane of section happens to pass so as to show only or chiefly the large nuclei. That I could mistake these for early cleavages was due to the fact that I was much more interested in the condition of the nuclei than in the course of cleavage, and my observations on the latter point were only incidental and so led to a wrong conclusion. Thus my error on this point is explained, but I do not offer the explanation as an excuse.

Furthermore, Richards is entirely correct in his assertion that during the earlier cleavages the blastomeres possess distinct boundaries and the egg is not a syncytial mass.

On the other hand my examination of Richards's material has enabled me to discover what seems to me a very important point that he has failed to note. In his material the early cleavages are, as he says, mitotic and the blastomeres distinct. The cytoplasm in these stages stains rather deeply and uniformly and is not vacuolated to any great extent. At certain stages, however, which may vary in different eggs to some extent, but at which the egg consists of a considerable number of blastomeres it undergoes a marked change in appearance. The cytoplasm loses its tingibility to a large extent and becomes highly vacuolated and the distinct boundaries of the blastomeres disappear. Apparently these boundaries disappear gradually,

for in many cases some blastomeres remain more or less distinct while others have completely disappeared and in still other cases the whole mass of the egg appears to be a syncytium. In my earlier studies of the cleavage I interpreted these stages as stages in the appearance rather than the disappearance of the cell membranes.

In these and later stages I find, both in Richards's material and in my own, the most striking evidence for the occurrence of amitosis that I have seen anywhere in *Moniezia*. Here the nuclei are large and they are not crowded, consequently they can be studied with less difficulty and a larger number of good cases can be readily found.

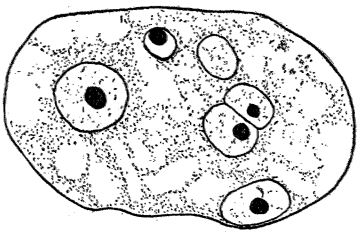
I give here a few figures drawn from Richards's slides to supplement my earlier figures.

In Fig. 9 five nuclei, one of them "double," are shown. No cell boundaries are visible and the cytoplasm is highly vacuolated and stains only slightly The two parts of the double nucleus are in immediate contact and the relation of the nucleoli to the dividing membrane and to each other which was mentioned above is seen.

Fig. 10 shows four nuclei, one of which appears to be dividing or to have recently divided. The two parts of this nucleus are of different size and are closely apposed.

Fig. 11 is an interesting case: the blastomere containing the double nucleus still retains its dense unvacuolated cytoplasm and to some extent its distinctness, though no actual cell membrane can be seen. The two parts of the nucleus which it contains are in immediate contact and flattened against each other. It is difficult to understand how they could attain such a position as the result of mitotic cleavage like that of the earlier stages. Moreover, in those earlier stages neither Richards nor I have found any evidence of the separation of paternal and maternal chromatin.

In Fig. 12 is shown a case of apparent division which seemed to me, as I examined it, particularly convincing. Here the two parts of the nucleus are of different size and a new nuclear membrane appears to be in process of formation between them. Its formation apparently began on one side and on that side the



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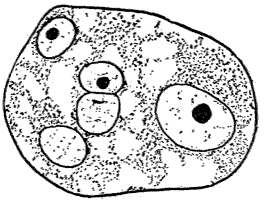

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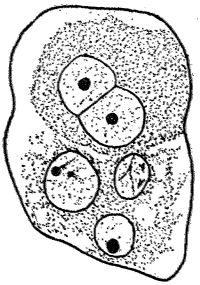

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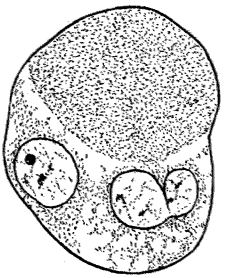

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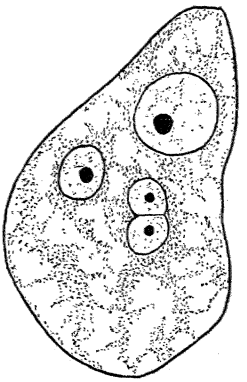

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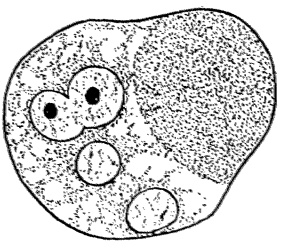

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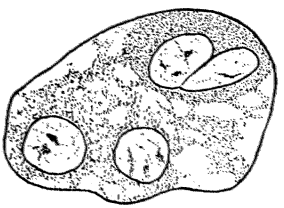

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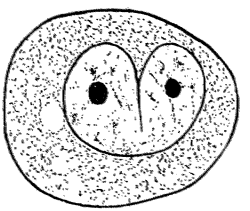

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nuclei have separated, while on the other side it is still very delicate and the nuclei are still closely connected. The dense mass in the upper part of the figure is the cytoplasm of a large blastomere which has not yet undergone the change in appearance; its nucleus is in another section.

Fig. 13 shows a section without any cell boundaries and with vacuolated cytoplasm in which one small nucleus is apparently dividing.

In Fig. 14 a constricted nucleus is shown. The cytoplasm of a large dense blastomere is also seen in the figure.

Fig. 15 shows another case which is to me very convincing. The double nucleus is in a mass of cytoplasm which is denser than the rest and still rather distinctly marked off from it. Evidently this represents a blastomere. Here, as in Fig. 12, the division of the nucleus apparently began from one side, for the membrane is much more distinct on the side where separation is most advanced and becomes more and more delicate toward the other side.

Fig. 16 represents a case of almost diagrammatic clearness in which a large nucleus is apparently dividing from one side. Other nuclei are in other sections of the egg.

Figures of this sort might be multiplied indefinitely from Richards's material as well as from my own. There is, I think, but little chance of observational error here, for the nuclei are relatively large and not crowded and in many cases almost diagrammatically distinct. If pictures of this sort are of any value as a criterion of the occurrence of amitosis, then the evidence is certainly very strong for these cleavage stages.

One question, however, must be raised here, viz., whether these are normal stages in development or degenerating embryos. To settle this point absolutely the study of later stages is necessary and thus far I have not been fortunate enough to obtain such stages. But in many proglottids practically every embryo shows this syncytial condition and if this indicates degeneration, then all, or almost all, the embryos of those proglottids are degenerating. It seems probable therefore that we have to do here with normal stages in the development.

III. Conclusion.

My examination of Richards's material has only confirmed me in my conclusion that direct division plays an important part in the developmental cycle of *Moniezia*, in the germ cells as well as in the soma. I believe I have at least shown clearly enough that this material contains something besides negative evidence for amitosis.

We are, I think, not yet acquainted with the nucleus as a dynamic system. We know something of it morphologically and almost all our hypotheses and theories concerning it are based on the morphological data obtained from fixed material.

But even when we consider only the results of direct observation it is difficult to understand how cytologists can continue as many do to ignore or minimize the importance of the rapidly increasing number of observations on the occurrence of direct division. Certainly the work of recent years on protozoa, to mention only this group, has brought to light many apparent facts which are not in agreement with current cytological theory. Where there is so much smoke it would seem that there must be some fire.

Moreover, when we regard the nucleus as a dynamic system rather than a morphological structure the necessity for assuming the individuality or continuity of the chromosomes disappears entirely. The reappearance of a definite and constant number of chromosomes in successive cell generations is essentially the same problem as the reappearance of five fingers on the hand in successive generations of man, or any other case of the inheritance of organs or parts in definite constant number yet we do not regard it as necessary to assume that these fingers or parts maintain their individuality at all stages of the life cycle. Instead of being the basis of heredity the chromosome is itself a problem in heredity. The theory of so-called chromosome individuality is a wholly unnecessary hypothesis and a heavy burden for cytology to carry, since it is not in accord with the facts and requires supplementary hypotheses at every point.

There is every reason to believe that the nucleus, like other dynamic systems, will behave differently under different internal and external conditions. What conceivable reason is there

to doubt that it can under certain conditions undergo fission or fragmentation and still retain the capacity to become a "whole" again? We know that the capacity for regulation is a very general characteristic of living things: are we justified in asserting without the strongest experimental evidence that the nucleus is wholly without this capacity?

If it were not for the fact that it makes no essential difference for the phenomena of life, including those of heredity, whether the chromosomes are continuous individuals or not this hypothesis would doubtless have been abandoned long ago. But since it is in a sense outside the field of scientific investigation, at least so long as present methods of cell study are in vogue, the chromosome theory has developed into a wonderful inverted pyramid of hypotheses in the construction of which the chromosome appears as a veritable *deus ex machina*.

The great need of cytology is a substitution of the experimental for the descriptive method: more experimentation and control and less inference from observation, a dynamic rather than a static point of view, rigorous proof rather than loose speculation; with such changes we may hope to gain some knowledge of the cell. The disadvantages of the present method are sufficiently manifest in the present paper.

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